



DEPARTMENT OF
HEALTH, EDUCATION & WELFARE

~~FEDERAL SECURITY AGENCY~~

PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

June 2, 1953

Communicable Disease Center
Enteric Bacteriology Laboratories
P. O. Box 185
Chamblee, Georgia

Dr. J. Lederberg
Department of Genetics
The University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg:

This will acknowledge your letter of May 29 and the copies of the manuscript. I shall not attempt to comment upon all the points raised in your letter but shall confine myself largely to answering questions in regard to the paper.

1. I approve of the course you have taken.
2. I shall submit three copies for approval.
3. I shall send copies to Kauffmann, Taylor, and Seligmann.
4. The form of the acknowledgements is satisfactory.
5. Please return the carbon I corrected. Otherwise, I am lost. I would like to go over the manuscript at leisure.
6. I believe the change on page 8, par. 3 should be made.
7. Page 12, par. 2 - This is a dilly. Change by all means.
8. I yield on the semisolid agar and the split verbs.
9. I suppose the reference to XII₂ is not too bad but I am convinced that, in general, it remains to be proven that phage receptors can be related to antigenic components.
10. I agree to the inclusion of *S. gallinarum*. You will have my note that 1043G2 removed all agglutinins from g,m serum. I shall not prepare 1043G2 serum until my return from Kentucky.

In regard to phase variation, I have no wish to be dogmatic. I must admit my own ideas have been replaced by doubts. However, there can be no doubt that SW1003 is much more variable in phase than SW726, from which it arose, using the same serums and medium. I still will send you the details. There is also the matter of transfer of V to SW1005. I am investigating the agglutinability (O) of these. (Kauffmann thinks he has transferred I to Durazzo XII₂).

In re the behavior of *S. abortus equi* in semisolid medium. It parallels that of *S. typhi* and reflects growth vigor (or lack of it) of

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the bacterium. We have learned to expect this.

Please note - I have no copy at all of the pedigree sheets sent to you. It is important that I have them.

I do not believe it worthwhile to publish the kunzendorf results. They were done for my own information. I advised John White to work on similar things for a doctorate thesis at Vanderbilt and would not want to cut the ground from under him. He has spent two years working unsuccessfully on O transformation and is slightly desperate. He also is working on S. abortus equi.

S. javiana occurs in 3 forms:

- a. Strains with a high mutation rate
- b. Strains with a low mutation rate
- c. Strains in which no variation can be demonstrated with serum and semisolid. This is not a matter of phase relationships (I think)

I have made a serum for your induced phase (SW977), do you want some of it? I have made j serum without d agglutinin - do you need any?

I know nothing about the Iseki papers but if they are no better than recent Japanese papers they are terrible. I would like to see them. I do not have the transformed strains but will ask Bruner if he has them.

n Serum - IV,XII: e,n,x absorbed by IV,V,XII: e,h.
Most cultures of S. abortus equi have to be transferred serially before they exhibit a phases.

I have no Para A O forms ~~no~~ do I have Roschka's S. typhi 0125.

I am quite confused by the Java behavior. I am still open for more work on this business and I promise to send you additional protocols.

With kind regards, I am

For the Officer-in-Charge, Bacteriology Section

Sincerely yours,

Phil

PRE:mg

Philip R. Edwards, Ph. D.
Bacteriologist-in-Charge
Enteric Bacteriology Unit